philosophy and science. Mr. Steele has done his work with much tact and care, and an interesting preface is contributed by Mr. William Morris.

Astronomy for Every-day Readers. By B. J. Hopkins, F.R.A.S. (London: George Philip and Son, 1893.)

THIS is a little book which aims at explaining in "as accurate and interesting a manner as possible such of the phenomena of the heavens as should be known to every intelligent person." It consists of six chapters dealing respectively with day and night, the phases of the moon, the tides, the seasons, eclipses, meteors, shooting stars, and comets. Descriptive astronomy is not touched upon, but there is an introductory chapter giving a general survey of the solar system and its dimensions. The book has been very carefully written, and the scientific explanations are much relieved by interesting references to the history of the subject. The author has succeeded in giving very clear and concise accounts of the every-day phenomena with which the book specially deals, and it seems well adapted to awaken a desire for more in the class of readers to whom he more particularly appeals. A biography of the author—who is described as "the working-man scientist"—is also included.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Blind Animals in Caves.

IN an article in the current number of the *Contemporary Review* Mr. Herbert Spencer discusses the "familiar instance" of blind animals in caves as bearing upon the hypothesis of the transmission of acquired characters. Mr. Spencer is not satisfied with the explanation of the blindness of these cave animals offered by Weismann, who endeavours to account for them by two conditions recognised as operating in regard to other cases by Darwin, viz. cessation of selection and parsimony of growth ("Origin of Species," sixth edition, p. 118), of which the former author has treated under the name Panmyxia. Mr. Spencer shows that the saving of ponderable material in the suppression of an eye is but a small economy: he loses sight of the fact, however, that possibly, or even probably, the saving to the organism in the reduction of an eye to a rudimentary state is not to be measured by mere bulk, but by the non-expenditure of special materials and special activities which are concerned in the production of an organ so peculiar and elaborate as is the vertebrate eye.

That, however, to which I wish here to draw the attention of Mr. Spencer and his readers is this:—Mr. Spencer appears to think that if he disposes of Weismann's explanation of the blindness of cave-animals according to "Panmyxia"—there remains only the explanation by "transmission of acquired characters" in the field. He appears not to be acquainted with the explanation which I have offered of the blindness of cave-animals. It is closely similar to that given by Darwin of the occurrence of wingless insects on oceanic islands. My explanation consists in an application to the case in hand of Darwin's principle of "natural selection." I published it some years ago in my article "Zoology" in the "Encycl. Britannica," reprinted in 1890 in a volume of essays, bearing the title "The Advancement of Science." My suggestion was (and is) as follows, and I should like to see what Mr. Spencer has to say to it:—"This instance (that of the blind cave-animals) can," I wrote in the article above-named, "be fully explained by natural selection acting on congenital fortuitous variations. Many animals are thus born with distorted or defective eyes whose parents have not had their eyes submitted to any peculiar conditions. Supposing a number of some species of Arthropod or Fish to be swept into a cavern or to be carried from less to greater depths in the sea, those individuals with perfect eyes would follow the glimmer of light and eventually escape to the outer air or the shallower depths, leaving behind those with imperfect eyes to breed in the dark place. A natural selection would thus be effected. In every succeeding generation (bred in the dark place) this would be the case, and even those with weak but still seeing eyes would in the course of time escape, until only a pure race of eyeless or blind animals would be left in the cavern or deep sea."

My own position in regard to the hypothesis of the transmission of acquired characters remains what it was ten years ago, viz. that in the absence of observed instances of this transmission and in the presence of repeated observation that particular acquired characters are *not* transmitted, I do not consider it legitimate to *assume* a transmission of acquired characters as the explanation of any given case, such, for instance, as that of the blind cave-animals. I am confirmed in this attitude by the fact that a little consideration has enabled me and others to explain satisfactorily, by reference to no hypothetical causes, but to the admitted and demonstrable facts of "congenital variation" and "natural selection," instances brought forward as "only to be explained on the assumption of the truth of Lamarck's hypothesis."

On the other hand, I have always considered that there is not sufficient ground for asserting that a transmission of acquired characters *can not* take place. The important question is still as it was five years ago, "*Does* it take place?"

Oxford, February 14. E. RAY LANKESTER.

Glacier Action.

I HAVE read with great interest and pleasure the short review in your paper of last week by Prof. Bonney, giving a summary of the results of a survey of the French freshwater lakes, and indicating as the most probable conclusion that they cannot be accounted for on the theory of the late Sir A. Ramsay, by the digging-out power of glaciers.

Living as I do in a highly glaciated country, and in a country also full of lakes, both fresh and salt, I have never believed in that theory. Lakes seem to me to be due to the same causes which have produced the glens and hollows in which they lie, and these causes cannot be identified with glacier action alone. The theory of Ramsay attributes to glacier action powers and effects which have never been proved to belong to them. Glaciers do not dig out. They rub down—abrade—and scoop, when they are moving down inclined planes at angles more or less steep. But when they reach level ground they do not dig; they rest upon the level surfaces, and when pressed from behind they flow over it. But I have never seen any proof that they can act like a ploughshare, or rather like one of the new digging machines.

In so far as all existing glens may have been formerly occupied by glaciers, their depths must have been increased by glacier action, on the supposition that they were tilted, or upraised at some angle required for this form of true glacier action. On this supposition, indeed, lake basins may be said to be partly due to glaciers. But then this supposition involves and depends upon the assumption that earth movements have made the lake basins what they now are—hollows in a comparative level.

Like all other general theories in the history of geology, the "glacial theory" seems to me to have been ridden to the death, and I have been long waiting for some signs of that reaction or correction which is still much needed. I hold that in this country there is not only no evidence of "ice sheets" overriding all the hills, but the strongest evidence against such sheets. Our glens had true glaciers in abundance, no doubt, and they have left their tool-marks very distinctly. But those marks are quite inconsistent with one universal ice-cap or ice-sheet over all the land. ARGYLL.

Inveraray, February 16.

Dr. Joule's Thermometers.

EVERY one will, I am sure, be glad to know that Dr. Joule's thermometers are under investigation by Prof. Schuster.

It is unfortunate that Joule does not give the actual readings of the freezing point, but if the comparison quoted by Rowland was made in either 1879 or 1890 it may be that he referred to the reading of November, 1879, when the total rise of the zero point was 12·92 scale divisions. In that case the original reading in April, 1844, would be 9·70; at any rate this number cannot be very far from the truth.

The temporary changes of zero point alluded to by Prof. Schuster certainly complicate the matter, but from the numbers given it would appear that since 1879 or 1880 there has been a

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further secular rise of from 0·38 to 0·89 of a scale division. Nothing is said in Joule's paper about the temperatures at which the thermometers had been kept before the readings of the freezing point were taken, but as the later observations—and most of the earlier ones—were made in the winter months, it may perhaps be assumed that the temperatures were nearer 7° than 30°, and that the actual reading on the scale last winter should be taken as nearer 23·51 than 23·00. If this is so the total rise of the zero point last winter would be nearer 13·81 than 13·30.

Prof. Schuster states that "with properly annealed thermometers the secular changes are much smaller than the temporary ones," and that is no doubt true for observations extending over a limited time and with such comparatively large variations of temperature as from 7° to 30°. It may be pointed out, however, that the secular rise since 1879 or 1880 is probably greater than the maximum temporary change recorded by Prof. Schuster, and of course the total secular rise is enormously greater.

It may be true that the secular changes of a thermometer gradually vanish, but it must, I think, be conceded, that in the case of Dr. Joule's thermometer it will be a long time before absolute constancy is attained. There can be no doubt that even now, nearly forty-nine years after the first reading was taken, the zero point is still rising, and it does not appear to me to be very improbable that during the next fifty years there may be a further rise of two scale divisions, the amount calculated from the purely empirical formula which I have suggested.

Sydney Young.

University College, Bristol, February 20.

Foraminifer or Sponge?

Under the above heading in last week's Nature Dr. Hanitsch briefly draws our attention to Mr. A. Goës' report on the deep sea organisms procured by Prof. Agassiz in the American tropical Pacific, which he describes as Arenaceous Foraminifera, with the name *Neusina Agassizi.*

As it was from me that Dr. Hanitsch received the specimens he describes, which I had after a personal conversation on the matter sent him, for his opinion as to their relation to true sponges, I venture to send some further observations on these interesting forms.

Dr. Hanitsch is, I believe, quite right in referring Mr. Goës' Neusina to Prof. Haeckel's *Stannophyllum zonarium*, as described in his report on the *Challenger* deep-sea Keratosa. But while admitting my admiration of Prof. Haeckel's wonderful production on the *Challenger* specimens, I do not agree with him as to their being true Keratose sponges.

My conclusion is based upon the examination of nearly the whole *Challenger* collection, and in not one species could I find the slightest trace of any of the flagellated chambers characteristic of sponges.

Prof. Haeckel accounts for the absence of this important feature through the bad preservation of the specimens. Yet he describes the most delicate parts of a commensal Hydroid in full, and was able to observe amœboidal cells, and the granulated sarcode bodies peculiar to all bottom living Foraminifera.

If, however, the forms described by Prof. Haeckel prove after all to be true Keratose sponges, the present state of our knowledge does not justify their separation from such recognised genera of Foraminifera, as Masonella, and Syringammina of the late Dr. G. Brady; Technitella, Haliphysema, and Marsipella of Canon Norman; or *Hyperammina palmiformis*, described by myself from the Faröe Channel, all which forms have the power of forming siliceous and chitinous skeletons.

Without going into further detail here it will be readily understood that I quite agree with Mr. Goës in placing these organisms among the Foraminifera, although it would have been better had he given us a clearer and more detailed description of his Neusina.

I had hoped to have published my personal observations on these most interesting organisms, but circumstances have prevented me doing so up to the present.

I for one would be glad if Dr. Hanitsch would give his opinion as to their supposed sponge structure, which he has not done in his previous letter.

F. G. Pearcey,
Late of the *Challenger* Expedition and Commission.

Owens College Museum, Manchester.

Colonial Meteorology.

On p. 363 of your last number your reviewer of the "Year-book of the Imperial Institute," after remarking that "climate certainly deserves better treatment," continues:—

"We do not think space would be wasted in giving the mean monthly temperatures and rainfall for the average year and for two extreme years, at a few representative stations in the larger colonies. This information cannot indeed be found in any existing books, but must be worked out from original records, which exist abundantly, and are rarely made available to practical workers."

I am afraid that the reviewer does not always read Nature, for you, sir, have on several occasions noticed my efforts in this direction, efforts which have gone on uninterruptedly for twenty years. As, now that you have taken the matter up, it is not improbable that some of the funds lavished on the Imperial Institute may be devoted to the subject, and my small organisation be swamped or superseded, I hope that you will, in justice to the directors of the various Colonial observatories who have helped me for so many years, and as some consolation for the entire ignorement of our organisation by your reviewer, allow me to give its history in the fewest words possible.

In 1873 I determined to try to publish monthly a table giving the principal climatic data for each synchronous month at widely spread stations over the entire British Empire. The leading idea was identity, so as to ensure comparability. I therefore prepared some blank forms and sent them with a circular letter to about twenty of our leading Colonial meteorologists. Every one without exception promised to help, and it says much for colonial climate to add that during the subsequent twenty years not more than five or six of my original correspondents have passed away.

During the period occupied in the transit of my request and of the replies thereto, I wrote a series of short articles pointing out the leading features, and as far as practicable the mean values, for the various stations, so that when we began publishing the monthly values, the departures from the mean could be recognised. These articles and the tables themselves from 1874 to 1881 appeared in *The Colonies* (subsequently *The Colonies and India*). When in 1882 that paper passed into other hands, the proprietors declined to publish the tables, and I began to insert them in the *Meteorological Magazine*, where they have appeared regularly month by month for the subsequent thirteen years. At the close of each year an extra table is given with a summary of the results for the year, and Nature has often done me the honour of quoting portions of these summaries.

I enclose copy of our last table, and though I know that to reproduce it would be to make a somewhat large demand upon your space, I feel that the work (wholly unpaid, be it remembered) of my Colonial friends during these past twenty years, claims some consideration and some recognition. You will see by the signatures that the authorities are the highest attainable.

G. J. Symons.

62, Camden Square, N.W., February 17.

Ozone.

With reference to a paragraph in Nature, p. 373, on observations of ozone in the atmosphere, and the paucity of observers and records, I may be allowed to state that I have collected sets in the North Atlantic and Pacific Oceans and Mediterranean. These have been taken by officers of the Royal Navy and mercantile marine at sea, and some of the records have been tabulated, and may be communicated to some society in due course. Moffatt's papers, made by Negretti and Zambra, have been used throughout, so the observations are all uniform and comparable.

W. G. Black.

Edinburgh, February 19.

LION-TIGER AND TIGER-LION HYBRIDS.

The Council of the Royal Zoological Society of Ireland entertain some hope that it will be possible to produce in their Gardens examples of hybrids or cross-breeds between the two largest species of cat, namely, the lion and tiger.

That such hybrids have been produced is a matter of historical record, and as the writer is particularly inter-

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